

Interactive comment on “Particle acidity and sulfate production during severe haze events in China cannot be reliably inferred by assuming a mixture of inorganic salts” by Gehui Wang et al.

Anonymous Referee #1

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The manuscript presents field and laboratory results of sulfate formation during winter haze conditions in China. It points to the potentially important role that organics play in aerosol acidity and hygroscopic growth in China. An important finding is that SO₂ oxidation by NO₂, a new mechanism proposed for Beijing winter haze, is significant only on oxalic acid as seed particles, but not on ammonium sulfate due to the strong acidity. This finding was not stressed in the earlier work of the authors (Wang et al. 2016, PNAS). The paper is well written for most parts and should be published in ACP after the following comments are addressed.

My main comment is the representativeness of the lab experiment for atmospheric con-

C1

ditions in winter haze in China. I would expect the organics and inorganic compounds are mixed in ambient aerosol particles, so there will not exist pure oxalic acid aerosol or pure ammonium sulfate aerosol as used in the chamber experiments. I would suggest the authors discuss this issue in the manuscript, particularly with regard to how their proposed mechanism should be adopted in models where aerosols are typically assumed internally mixed.

Specific comments: Figure 1 shows no particle growth with ammonium sulfate as the seed particle. It would be convenient to compare the same growth chart with oxalic acid as the seed particle.

Figure 2: the graph shows growth of oxalic acid seed particles in the chamber experiments at different RH. How much of the growth is due to water and how much is it due to sulfate formation?

Figure 3. Please add sulfate time series and discuss if sulfate correlates with WSOC and/or oxalic acid.

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C2